May 5, 1881.

THE PRESIDENT (followed by THE TREASURER) in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of Candidates recommended for election into the Society were read from the Chair, as follows:—

Ayrton, Prof. William Edward.
Bates, Henry Walter.
Bristowe, John Syer, M.D., F.R.C.P.
Christie, William Henry Mahoney, M.A., Sec. R.A.S.
Dickie, Prof. George, A.M., M.D., F.L.S.
Kempe, Alfred Bray, B.A.
Macalister, Prof. Alexander, M.D., Sec. R.I.A.
McLeod, Prof. Herbert, F.I.C., F.C.S.
Phillips, John Arthur.
Preece, William Henry, C.E.
Samuelson, Bernhard, M.I.C.E.
Stoney, Bindon Blood, M.A., M.I.C.E.
Traquair, Ramsay H., M.D.
Watson, Rev. Henry William, M.A.
Wright, Charles R. Alder, D.Sc.

The following Papers were read:—

I. "On the Determination of the Ohm in Absolute Measure." By Lord RAYLEIGH, F.R.S., and ARTHUR SCHUSTER, Ph.D., F.R.S. Received April 12, 1881.

Part I.—By Lord RAYLEIGH.

It is generally felt that considerable uncertainty still attaches to the real value of the ohm, or British Association unit of resistance. The ohm was constructed to represent 10^9 C.G.S. absolute units, but according to Kohlrausch* it is nearly 2 per cent. too great, and according to Rowland† nearly 1 per cent. too small. On the other hand, H. Weber‡ has obtained by more than one method results very nearly in harmony with those of the British Association Committee. Influenced partly by the fact that the original apparatus (though a good deal out of repair) and the standard coils themselves were in

* "Phil. Mag.," xlvii, p. 294, 1874.
† "American Journal of Science and Arts," 1878.
‡ "Phil. Mag.," v, p. 30, 1878.

the Cavendish Laboratory, I determined last June to repeat the measurement by the method of the Committee, which has been employed by no subsequent experimenter, and sought permission from the Council of the British Association to make the necessary alterations in the apparatus. In this way I hoped not merely to obtain an independent result, but also to form an opinion upon the importance of certain criticisms which have been passed upon the work of the Committee.

The method, it will be remembered, consists in causing a coil of insulated wire, forming a closed circuit, to revolve about a vertical axis, and in observing the deflection from the magnetic meridian of a magnet suspended at its centre, the deflection being due to the currents developed in the coil under the influence of the earth's magnetism. The amount of the deflection is independent of the intensity of the earth's magnetic force, and it varies inversely as the resistance of the circuit. The theory of the experiment is explained very fully in the reports of the Committee,* and in Maxwell's "Electricity and Magnetism," section 763. For the sake of distinctness, and as affording an opportunity for one or two minor criticisms, a short statement in the original notation will be convenient:—

H=horizontal component of earth's magnetism.

γ=strength of current in coil at time t.

G=total area inclosed by all the windings of the wire.

ω=angular velocity of rotation.

$\theta=\omega t$=angle between plane of coil and magnetic meridian.

M=magnetic moment of suspended magnet.

ϕ=angle between the axis of the magnet and the magnetic meridian.

K=magnetic force at the centre of the coil due to unit current in the wire.

L=coefficient of self-induction of coil.

R=resistance of coil in absolute measure.

$MH\tau$=force of torsion of fibre per unit of angular rotation.

The equation determining the current is—

$$L\frac{d\gamma}{dt}+R\gamma=HG\omega\cos\omega t+MK\omega\cos(\omega t-\phi) \quad . \quad . \quad . \quad . \quad (1),$$

whence

$$\gamma=\frac{\omega}{R^2+L^2\omega^2}\{GH(R\cos\theta+L\omega\sin\theta)$$
$$+KM(R\cos(\theta-\phi)+L\omega\sin(\theta-\phi))\} \quad . \quad (2).$$

* Collected in one volume. London, 1873.

If L were zero, or if the rotation were extremely slow, the current would (apart from KM) be greatest when the coil is passing through the meridian. In consequence of self-induction, the phase of the current is retarded, and its maximum value is diminished. At the higher speeds used by the Committee, the retardation of phase amounted to 20°.

To find the effect of (2) upon the suspended needle, we have to introduce MK and the resolving factor $\cos(\theta-\phi)$, and then to take the average. This, on the supposition that the needle remains on the whole balanced at ϕ, must be equal to the force of restitution due to the direct action of the earth's magnetism and to torsion, *i.e.*, $\mathrm{MH}\sin\phi+\mathrm{MH}\tau\,\phi$. Thus—

$$\frac{\frac{1}{2}\mathrm{MK}\omega}{\mathrm{R}^2+\mathrm{L}^2\omega^2}\{\mathrm{GH}(\mathrm{R}\cos\phi+\mathrm{L}\omega\sin\phi)+\mathrm{KMR}\}-\mathrm{MH}(\sin\phi+\tau\,\phi)=0.$$

In the actual experiment τ is a very small quantity, say $\frac{1}{1000}$; and the distinction between $\tau\,\phi$ and $\tau\sin\phi$ may be neglected.

$$\mathrm{R}^2-\mathrm{R}\frac{\frac{1}{2}\mathrm{GK}\omega\cot\phi}{1+\tau}\left(1+\frac{\mathrm{MK}}{\mathrm{GH}}\sec\phi\right)+\mathrm{L}^2\omega^2-\frac{\frac{1}{2}\mathrm{GKL}\omega^2}{1+\tau}=0 \quad . \quad . \quad (3).$$

If we omit the small terms depending upon τ and upon MK/GH, we get on solution and expansion of the radical—

$$\mathrm{R}=\tfrac{1}{2}\mathrm{GK}\omega\cot\phi\left\{1-\frac{2\mathrm{L}}{\mathrm{GK}}\left(\frac{2\mathrm{L}}{\mathrm{GK}}-1\right)\tan^2\phi-\left(\frac{2\mathrm{L}}{\mathrm{GK}}\right)^2\left(\frac{2\mathrm{L}}{\mathrm{GK}}-1\right)^2\tan^4\phi\ldots\right\}$$
$$. \quad . \quad . \quad . \quad (4).$$

The term in $\tan^4\phi$ is not given in the report of the Committee, but, as I learn from Mr. Hockin through Dr. Schuster, it was included in the actual reductions. But the next term in $\tan^6\phi$, and one arising from a combination of the correction for self-induction with that depending on M, are not altogether insensible, so that probably the direct use of the quadratic is more convenient than the expansion. At the high speeds used by the Committee the correction for self-induction amounted to some 8 per cent., and therefore cannot be treated as very small.

If the axis of rotation be not truly vertical, a correction for level is necessary. In the case of coincidence with the line of dip, no currents, due to the earth's magnetism, would be developed. If the upper end of the axis deviate from the vertical by a small angle β towards the north, the electromotive forces are increased in the ratio $\cos(\mathrm{I}+\beta):\cos\mathrm{I}$, *i.e.*, in the ratio $1+\tan\mathrm{I}.\beta$, I being the angle of dip. A deviation in the east and west plane will have an effect of the second order only. The magnetic forces due to the currents will not act upon the needle precisely as if the plane of the coil were always vertical, but the difference is of the second order, so that the whole effect of a

small error of level may be represented by writing G $(1+\tan \mathrm{I}\,.\,\beta)$ for G in (3) or (4).

The next step is to express GK in terms of the measurements of the coil. In order that there may be a passage for the suspending fibre and its enveloping tube, it is necessary that the coil be double, or if we prefer so to express it, that there be a gap in the middle. If

a=mean radius of each coil,

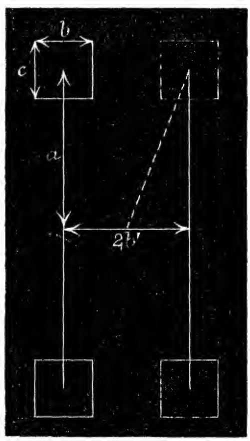
n=whole number of windings,

b=axial dimension of section of each coil,

c=radial dimension of section of each coil,

b'=distance of mean plane of each coil from the axis of motion,

α=angle subtended at centre by radius of each coil, so that $\cot \alpha = b'/a$,



then—

$$\mathrm{G}=\pi n a^2\left(1+\tfrac{1}{12}\frac{c^2}{a^2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5),$$

$$\mathrm{K}=\frac{2\pi n}{a}\sin^3\alpha\left\{1+\tfrac{1}{24}\frac{c^2}{a^2}(2-15\sin^2\alpha\cos^2\alpha)\right.$$
$$\left.+\tfrac{1}{24}\frac{b^2}{a^2}(15\sin^2\alpha\cos^2\alpha-3\sin^2\alpha)\right\} \quad . \quad (6),$$

so that

$$\mathrm{GK}=2\pi^2 n^2 a\sin^3\alpha\left\{1+\tfrac{1}{6}\frac{c^2}{a^2}+\tfrac{5}{8}\frac{b^2-c^2}{a^2}\sin^2\alpha\cos^2\alpha-\tfrac{1}{8}\frac{b^2}{a^2}\sin^2\alpha\right\} \quad . \quad (7).$$

The correction due to the finiteness of b and c is in practice extremely small, but the factor $\sin^3\alpha$ must be determined with full accuracy.

In order to arrive at the value of MK/GH, which occurs in (3), we observe that the approximate value of K/G is $2\sin^3\alpha/a^3$; so that

MK/GH is equal to $\tan\mu$, where μ is the angle through which the needle of a magnetometer is deflected when the suspended magnet (M) is placed at a distance from it $a/\sin a$ to the east or west, with the magnetic axis pointing east or west. In practice the difference of readings when M is reversed is taken in order to double the effect, and any convenient distance is used in lieu of $a/\sin\alpha$, allowance being easily made by the law of cubes.

The correction for torsion is determined by giving the suspended magnet one (or more) complete turns, and observing the displacement. If this be δ_1, reckoned in divisions of the scale, *i.e.*, in millimetres, and D be the distance from the mirror to the scale reckoned in millimetres,

$$\tau = \frac{\delta_1}{4\pi D} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

The correction for scale reading, necessary in order to pass from $\frac{1}{2}\tan 2\phi$ to $\tan\phi$, will be explained under the head of reductions.

Corrections depending upon irregularity in the magnetic field, and in the adjustment of the magnet to the centre of the coil are given in the report. They are exceedingly small. The same may be said of errors due to imperfect adjustment of the coil with respect to the axis of rotation.

In remounting the apparatus the first point for consideration was the driving gear. The Committee used a Huyghens' gearing, driven by hand, in conjunction with a governor. This, it appeared to me, might advantageously be replaced by a water-motor; and Bailey's "Thirlmere" engine, which acts by the impulse of a jet of water upon revolving cups, was chosen as suitable for the purpose. As the pressure in the public water pipes is not sufficiently uniform, it was at first intended to introduce a reducing valve; but on reflection it seemed simpler to obtain a constant head of water by connecting the engine with a small cistern at the top of the building. This cistern is just big enough to hold the ball-tap by which it is supplied, and gives at the engine a head of about 50 feet.

The success of this arrangement depends upon attention to principles, as to which it may be well to say a few words. The work done by many prime movers is within practical limits proportional to the speed. If the work necessary to be done in order to overcome resistances, as n overcoming solid friction, or in pulling up weights, be also proportional to the speed, there is nothing to determine the rate of the engine, and in the absence of an effective governor the motion will be extremely unsteady. In general the resistance function will be of the form—

$$Bv + Cv^2 + Dv^3 + \ . \ . \ . \ ,$$

in which the above-mentioned resistances are included under B. The

term in C will represent resistances of the nature of viscosity, and that in D a resistance such as is incurred in setting fluids in motion by a fan or otherwise. By these resistances, if present, the speed of working will be determined.

In the water impulse engine, however, the work is not proportional to the speed. At zero speed no work is done; neither is any work done at a speed such that the cups retreat with the full velocity of the jet. The speed of maximum efficiency is the half of the last, and the curve representing work as a function of speed is a parabola with vertex directed upwards. If we draw upon the same diagram the curve of work and the curve of resistances, the actual speed will correspond to the point of intersection, and will be well or ill defined according as the angle of intersection is great or small. At the higher speeds of the coil (four to six revolutions per second) so much air is set in motion that the resistance curve is highly convex downwards, and no difficulty is experienced in obtaining a nearly uniform motion. But when the speed of rotation is as slow as once a second, the principal resistance is due to solid friction, and the requisite curvature in the diagrams must be obtained in the curve of work. It was necessary in order to obtain a satisfactory performance at low speeds to introduce an additional reducing pulley, so that the engine might run fast, although the coil was running slow.

The revolving coil with its frame, and the apparatus for suspending the magnet, were at first arranged as described by the Committee. This description, with drawings, is to be found in the report, and it is reproduced in "Gordon's Electricity and Magnetism," vol. i. The water engine was ready about the middle of June, and towards the end of the month the apparatus was mounted by Mr. Horace Darwin. During July and August preliminary trials were made by Mr. Darwin, Mrs. Sidgwick, and myself, and various troubles were encountered.

The only point in which the arrangement adopted by the Committee was intentionally departed from was in the connexion of the magnet and mirror. The magnet is necessarily placed at the centre of the revolving coil, but in their arrangement the mirror is on the top of the frame and is connected to the magnet by a brass wire. In order to save weight, I preferred to have the magnet and mirror close together, not anticipating any difficulty from the periodic and very brief interruption caused by the passage of the coil across the line of sight. A box was, therefore, prepared with a glass front, through which the mirror could be observed, and was attached to the end of a brass tube coming through the hollow axle of the coil. This tube itself was supported on screws resting on the top of the frame. The upper end of the suspension fibre was carried by a tall tripod resting independently on the floor.

The first matter for examination was the behaviour of the magnet

and mirror when the coil was spinning with circuit open. At low speeds the result was fairly satisfactory, but at six or more revolutions per second a violent disturbance set in. This could not be attributed to the direct action of wind, as the case surrounding the suspended parts was nearly air-tight, except at the top. It was noticed by Mr. Darwin that even at low speeds a disturbance was caused at every stroke of the bell. This observation pointed to mechanical tremor, communicated through the frame, as the cause of the difficulty, and the next step was to support the case surrounding the suspended parts independently. A rough trial indicated some improvement, but at this point the experiments had to be laid aside for a time.

From the fact that the disturbance in question was produced by the slightest touch (as by a tap of the finger nail), upon the box, while the upper parts of the tube could be shaken with impunity, it appeared that it must depend upon a reaction between the air included in the box and the mirror. It is known that a flat body tends to set itself across the direction of any steady current of the fluid in which it is immersed, and we may fairly suppose that an effect of the same character will follow from an alternating current. At the moment of the tap upon the box the air inside is made to move past the mirror, and probably executes several vibrations. While these vibrations last, the mirror is subject to a twisting force tending to set it at right angles to the direction of vibration. The whole action being over in a time very small compared with that of the free vibrations of the magnet and mirror, the observed effect is as if an impulse had been given to the suspended parts.

In order to illustrate this effect I contrived the following experiment.* A small disk of paper, about the size of a sixpence, was hung by a fine silk fibre across the mouth of a resonator of pitch 128. When a sound of this pitch is excited in the neighbourhood, there is a powerful rush of air in and out of the resonator, and the disk sets itself promptly across the passage. A fork of pitch 128 may be held near the resonator, but it is better to use a second resonator at a little distance in order to avoid any possible disturbance due to the neighbourhood of the vibrating prongs. The experiment, though rather less striking, was also successful with forks and resonators of pitch 256.

It will be convenient here to describe the method adopted for regulating and determining the speed of rotation, which has proved thoroughly satisfactory. In the experiments of the Committee a governor was employed, and the speed was determined by means of the bell already referred to. This bell received a stroke every 100 revolutions, and the times were taken with a chronometer. In this

* "Proc. Camb. Phil. Soc.," Nov. 8, 1880.

method rather long spinnings (ten or twenty minutes) are necessary in order to get the speed with sufficient accuracy, much longer than are required to take the readings at the telescope. Desirous, if possible, of making the observations more quickly, I determined to try the stroboscopic method. On the axis of the instrument a stout card of 14 inches diameter was mounted, divided into concentric circles of black and white teeth. The black and white spaces were equal, and the black only were counted as teeth. There were five circles, containing 60, 32, 24, 20, 16 teeth respectively, the outside circle having the largest number of teeth.

This disk was observed from a distance through a telescope, and an arrangement for affording an intermittent view. An electric tuning fork of frequency about $63\frac{1}{2}$ was maintained in regular vibration in the usual way by means of a Grove cell. To the ends of the prongs are attached thin plates of metal, perforated with somewhat narrow slits parallel to the prongs. In the position of equilibrium these slits overlap so as to allow an unobstructed view, but in other positions of the fork the disk cannot be seen. When the fork vibrates, the disk is seen intermittently 127 times a second; and if the speed be such that on any one of the circles 127 teeth a second pass a fixed pointer, that circle is seen as if it were at rest.

By means of the various circles it is possible to observe correspondingly varied speeds without any change in the frequency of the fork's vibration. A further step in this direction may be taken by modifying the arrangement for intermittent view. If the eye be placed at the top or bottom of one of the vibrating plates, a view is obtained once only, instead of twice, during each vibration of the fork. This plan was adopted for the slowest rotation, and allowed 60 teeth to take the place of 120, which would otherwise have been necessary.

The performance of the fork was very satisfactory. It would go for hours without the smallest attention, except an occasional renewal of the alcohol in the mercury cup. Pure (not methylated) alcohol was used for this purpose, and a *platinum* point made and broke the contacts. Although, as it turned out, this fork vibrated with great regularity, dependence was not placed upon it, but repeated comparisons by means of beats were made between it and a standard fork of Kœnig's construction, of pitch (about) 128. These beats, at pitch 128, were about 48 per minute, and scarcely varied perceptibly during the course of the experiments. They could have been counted for an even longer time, but this was not necessary. It was intended at first to make the comparisons of the fork simultaneous with the other observations, but this was given up as a needless refinement.

Some care was necessary in the optical arrangements to obviate undue fatigue of the eyes in a long series of observations. In daylight the illumination of the card was sufficient without special provision,

but at night, when the actual observations were made, the image of an Argand gas flame was thrown upon the pointer and the part of the card near it. On account of the necessity of removing the electric fork and its appliances to a distance, the card, if looked at directly, would appear too much fore-shortened, and a looking-glass was therefore introduced. The eyepiece of the telescope, close in front of the slits, was adjusted to the exact height, and the eye was placed immediately behind the slits. By cutting off stray light as completely as possible, the observation may be made without fatigue and with slits narrow enough to give good definition when the speed is correct.

As governor I had originally intended to employ an electro-magnetic contrivance, invented a few years ago by La Cour and myself,* in which a revolving wheel is made to take its time from a vibrating fork, and it was partly for this reason that the water engine was placed at a considerable distance from the revolving coil. I was, however, not without hopes that a governor would be found unnecessary, and a few trials with the stroboscopic apparatus were very encouraging. It appeared that by having the water power a little in excess, the observer looking through the vibrating slits could easily control the speed by applying a slight friction to the cord connecting the engine and coil. For this purpose the cord was allowed to run lightly through the fingers, and after a little practice there was no difficulty in so regulating the speed that a tooth was never allowed finally to pass the pointer, however long the observation was continued. If, from a momentary inadvertence or from some slight disturbance, a tooth passed it could readily be brought back again. The power of control thus obtained will be appreciated when it is remembered that the passage of a tooth *per second* would correspond to less than one per cent. on the speed. In many of the observations the pointer covered the same tooth all the while, so that the introduction of a governor could only have done harm.

Another, and perhaps still more important, improvement on the original method related to the manner of making correction for the changes of declination which usually occur during the progress of the experiments. The Committee relied for this purpose upon comparisons with the photographic records made at Kew, and they recognise that considerable disturbances arose from the passage of steamers, &c. All difficulty of this kind is removed by the plan which we adopted of taking simultaneous readings of a second magnetometer, called the auxiliary magnetometer, placed at a sufficient distance from the revolving coil to be sensibly unaffected by it, but near enough to be similarly influenced by changes in the earth's magnetism, and by other disturbances having their origin at a moderate distance. The

* "Nature," May 23, 1878.

auxiliary magnetometer was of very simple construction, and was read with a telescope and a millimetre scale, the distance between mirror and scale (about $2\frac{1}{2}$ metres) being adjusted to approximate equality with that used for the principal magnet, so that disturbances were eliminated by simple comparisons of the scale readings. During a magnetic storm it was very interesting to watch the simultaneous movements of the magnets.

In the month of September the apparatus was remounted under the direction of Professor Stuart, to whose advice we have often been indebted. In order to examine whether any errors were caused by the circulation of currents in the frame, as has been suggested by more than one critic, insulating pieces were inserted, mercury cups at the same time being provided, so that the contacts could be restored at pleasure. But the principal changes related to the manner of suspending the fibre and supporting the box and tube. In order to eliminate tremor, as far as possible, these parts were supported by a massive wooden stand, resting on the floor and overhanging, but without contact, the top of the metal frame of the coil. The upper end of the fibre was fastened to a rod sliding in a metal cap, which formed the upper extremity of a 2-inch glass tube. Near the other end this tube was attached to a triangular piece of brass, resting on three screws, by which the whole could be raised or lowered bodily and levelled. Rigidly attached to this tube, and forming a continuation of it, a second glass tube, narrow enough to pass freely through the hollow axle of the coil, protected the fibre as far as the box in which the mirror and magnet were hung. This box was cylindrical and about 3 inches in diameter. The top fitted stiffly to the lower end of the narrow glass tube, and the body of the box could be unscrewed, so as to give access to the interior. The window necessary for observation of the mirror was made of a piece of worked glass, and was fitted air-tight.

On my return to Cambridge in October the apparatus was tested, but without the full success that had been hoped for. At high speeds there was still unsteadiness enough to preclude the use of these speeds for measurement. Since it is impossible to suppose that the tremor is propagated with sufficient intensity through the floor and massive brickwork on which the coil is supported, the cause must be looked for in the fanning action of the revolving coil, aggravated no doubt by the somewhat pendulous character of the box, and perhaps by the nearness of the approach between the coil and its frame at three points of the revolution.

At this time the experiment was in danger of languishing, as other occupations prevented Mr. Darwin from taking any further part; but on Dr. Schuster's return to Cambridge he offered his valuable assistance. Encouraged by Sir W. Thomson, we determined to proceed with

the measurements, inasmuch as no disturbance, due to the rotation of the coil with circuit open, could be detected until higher speeds were approached than it was at all necessary to use.

One of the first points submitted to examination was the influence of currents induced in the frame. Without altering the speed or making any other change, readings were taken alternately with the contact-pieces in and out. Observations made on several days agreed in showing a small effect, due to the currents in the frame, in the direction of a *diminished* deflection. The whole deflection being 516 divisions of the scale, the mean diminution on making the top contacts was ·86 division. When the coil was at rest no difference in the zero could be detected on moving the contact-pieces.

In these preliminary experiments very consistent results were obtained at constant speeds, whether the rotation was in one direction or the other; but when deflections at various speeds were compared, we were startled to find the larger deflections falling very considerably short of proportionality to the speeds. There are only two corrections which tend to disturb this proportionality—(1) the correction for scale-reading, (2) the correction for self-induction. The effect of the first is to make the readings too high, and of the second to make the readings too low at the greater speeds. According to the figures given by the Committee (Report, p. 106), the aggregate effect is to increase the readings, on account of the preponderance of (1) over (2), whereas our results were consistently of the opposite character. Everything that could be thought of as a possible explanation was examined theoretically and experimentally, but without success. The coil was dismounted and the wire unwound, in order to see whether there was any false contact which might be supposed to vary with the speed and so account for the discrepancy. After much vexation and delay, it was discovered that the error was in the statement in the Report, the effect of self-induction being given at nearly ten times less than its real value. The correction for scale-reading, instead of preponderating over the correction for self-induction, is in reality quite a small part of the whole.

At this stage, as time was running short, we determined to proceed at once to a complete series of readings at sufficiently varied speeds, postponing the measurement of the coil to the end. The wire had been rewound without extreme care to secure the utmost attainable evenness, and the condition of the groove was such that a thoroughly satisfactory coil could not have been obtained, even with extreme care. It appeared, however, on examination that irregularities of this sort were not likely to affect the final result more than one or two parts in a thousand, if so much; and many points of interest could be decided altogether independently of this measurement.

The details of the experiments and reductions are given below by

Dr. Schuster, who took all the readings of the principal magnetometer. Mrs. Sidgwick observed the auxiliary magnetometer; while the regulation of the speed by stroboscopic observation fell to myself. Dr. Schuster also undertook the labour of the reductions and the final comparisons of our arbitrary German silver coil with the standard ohms.

The observations were very satisfactory, and at constant speeds agreed better than we had expected. The only irregularity that we met with was a slight disturbance of the zero, due to convection currents in the air surrounding the mirror, the effect of which, however, almost entirely disappears in the means. This disturbance could be magnified by bringing a paraffin lamp into the neighbourhood of the mirror. After about half a minute, apparently the time occupied in conduction through the box and in starting the current, the readings began to move off. Complete recovery would occupy twenty or thirty minutes. In future experiments this kind of disturbance will be very much reduced by increasing the moment of the magnet five or six times, and by diminishing the size of the mirror, both of which may be done without objection.

The comparison of the results at various speeds requires a knowledge of the cœfficient of self-induction L. Nothing is said in the Report as to the value of L for the second year's experiments, but the missing information is supplied in Maxwell's paper on the "Electro-magnetic Field,"* together with an indication of the process followed in calculating it. The first approximation to the value of L, in which the dimensions of the section are neglected in comparison with the radius of the coil, is 437,440 metres, but this is reduced by corrections to 430,165. The value which best satisfies the observations is considerably greater, viz., 456,748. A rough experiment with the electric balance gave 410,000; but Professor Maxwell remarks that the value calculated from the dimensions of the coil is probably much the more accurate, and was used in the actual reductions. I had supposed at one time that the discrepancy between the results at various speeds and the calculated value of L was due to the omission of the term in $\tan^4 \phi$, given above, which would have the same general effect as an under-estimate of L; but, as has been already mentioned, this term was in fact included in the reductions made by Mr. Hockin, in conjunction, moreover, with the value L=437,440.

A rough preliminary reduction of our observations showed at once that they could not be satisfied by any such value of L as 437,000, but pointed rather to 454,000, and we began to suspect that the influence of self-induction had been seriously under-estimated by the Committee. Preliminary trials by Maxwell's method with the electric

* "Phil. Trans.," 1865.

balance giving promise of results trustworthy within one per cent., we proceeded to apply it with care to the determination of L, but the galvanometer at our command—a single needle Thomson of 2,000 ohms resistance—was not specially suitable for ballistic work. As this method is not explained in any of the usual text-books, it may be convenient here to give a statement of it.

The arrangement is identical with that adopted to measure the resistance of the coil in the usual way by the bridge. If P be the resistance of the copper coil, Q, R, S, nearly inductionless resistances from resistance-boxes, balance is obtained at the galvanometer when $PS=QR$. This is a resistance balance, and to observe it the influence of induction must be eliminated by making the battery contact a second or two before making the galvanometer contact. Let us now suppose that P is altered to $P+\delta P$. The effect of this change would be annulled by the operation of an electromotive force in branch P of magnitude $\delta P.x$, where x denotes the magnitude of the current in this branch before the change. Since electromotive forces act independently, the effect upon the galvanometer of the change from P to $P+\delta P$ is the same as would be caused by $\delta P.x$ acting in branch P, if there be no E.M.F. in the battery branch at all.

Returning now to resistance P, let us make the galvanometer contact before making the battery contact. There is no permanent current through the galvanometer (G), but, at the moment of make, self-induction opposes an obstacle to the development of the current in P, which causes a transient current through G, showing itself by a throw of the needle. The integral magnitude of this opposing E.M.F. is simply Lx, and it produces the same effect upon G as if it acted by itself. We have now to compare the effects of a transient and of a permanent E.M.F. upon G. This is merely a question of galvanometry. If T be the time of half a complete vibration of the needle, θ the permanent deflection due to the steady E.M.F., α the throw due to the transient E.M.F., then the ratio of the electromotive forces, or of the currents, is

$$\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\theta}.$$

If, instead of the permanent deflection θ, we observe the first throw (β) of the galvanometer needle, this becomes

$$\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\frac{1}{2}\beta}.$$

In the present case, the ratio in question is, by what has been shown, above $\delta P.x : Lx$, or $\delta P : L$; so that

$$\frac{L}{P}=\frac{\delta P}{P}\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\frac{1}{2}\beta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9),$$

a formula which exhibits the time-constant of the coil P in terms of the period of the galvanometer needle. Further to deduce the value of L in absolute measure from the formula requires a knowledge of resistances in absolute measure.

In carrying out the experiment the principal difficulty arose from want of permanence of the resistance balance, due to changes of temperature in the copper coil. The error from this source was, however, diminished by protecting the coil with flannel, and was in great measure eliminated in the reductions. The result was $L=455{,}000$ metres. This is on the supposition that the ohm is correct. If, as we consider more probable, the ohm is one per cent. too small, the result would be $L=450{,}000$.

Without attributing too great importance to this determination, there were now three independent arguments pointing to the higher value of L: first, from the experiments of the Committee; secondly, and more distinctly, from our experiments; and thirdly, from the special determination; and I entertained little doubt that a direct calculation from the dimensions of the coil would lead to a similar conclusion.

This direct calculation proved no very easy matter. Mr. W. D. Niven (whom I was fortunately able to interest in the question) and myself had no difficulty in verifying independently the formulæ given in "Maxwell's Electricity and Magnetism," §§ 692, 705, from which the self-induction of a simple coil of rectangular section can be found, on the supposition that the dimensions of the sections are very small in comparison with the radius. In the notation of the paper on the electro-magnetic field, if r be the diagonal of the section, and θ the angle between it and the plane of the coil,

$$L=4\pi n^2 a\Big[\log_e\frac{8a}{r}+\tfrac{1}{12}-\tfrac{4}{3}(\theta-\tfrac{1}{4}\pi)\cot 2\theta-\tfrac{1}{3}\pi\operatorname{cosec} 2\theta \\ -\tfrac{1}{6}\cot^2\theta\log_e\cos\theta-\tfrac{1}{6}\tan^2\theta\log_e\sin\theta\Big] \quad . \quad . \quad . \quad (10).$$

In the paper itself, probably by a misprint, $\cos 2\theta$ appears, instead of $\operatorname{cosec} 2\theta$, in (10). The expression is, as it evidently ought to be, unchanged when $\frac{1}{2}\pi-\theta$ is written for θ. By an ingenious process, explained in the paper, the formulæ is applied to calculate the self-induction of a double coil.*

The whole self-induction of the double coil is found by adding together twice the self-induction of each part and twice the mutual induction of the two parts. The self-induction of each part is found (to

* The following misprints may be noticed:—

Page 509, line 11, for B read C.
" " 13, for L(AC) read M(AC).
" " for L(B) read L(C).

Attention must be directed to the peculiar meaning attached to *depth*.

this approximation) by a simple application of (10). For twice this quantity Mr. Niven found 301,802, and I found 301,920 metres. For twice the mutual induction of the two parts I found, by Maxwell's method, 145,820 metres. Adding 301,920 and 145,820, we get 447,740 metres as the value of the whole self-induction, on the supposition that the curvature may be neglected. This corresponds to the value 437,440 given in the paper.

As to the origin of the discrepancy I am not able to offer any satisfactory explanation. It should be noticed, however, that owing to his peculiar use of the words "depth" and "breadth" as applied to coils, Maxwell has interchanged what, to avoid any possible ambiguity, I have called the *axial* and *radial* dimensions of the section. Thus the depth, *i.e.*, in his use of the word, the axial dimension, is given as ·01608, but this is really the radial dimension, as appears clearly enough from the Report of the Committee, as well as from our recent measurements. The real value of the axial dimension is ·01841 metre. But I do not think that this interchange will explain the difference in the results of the calculation.

When we proceed to apply corrections for the finite size of the section, further discrepancies develope themselves. The second term in the expression for L given in the paper (p. 508) does not appear to be correct, and the final numerical correction for curvature ($-7,345$ metres) differs in sign from that which we obtain. Mr. Niven has attacked the problem of determining the correction for curvature in the general case of a single coil of rectangular section, and (subject to a certain difficulty of interpretation) has obtained a solution. The application of the result to the actual case of a double coil would, however, be a very troublesome matter. For the two particular cases in which only one of the two dimensions of the section of a simple coil is considered to be finite, Mr. Niven and myself have independently obtained tolerably simple results. Thus, if the axial dimension be zero ($b=0$),

$$\mathrm{L}=4\pi n^2 a\left[\log\frac{8a}{c}-\tfrac{1}{2}+\frac{c^2}{96a^2}\left(\log\frac{8a}{c}+\tfrac{43}{12}\right)\right] \quad . \quad . \quad (11);$$

and if the radial dimension be zero,

$$\mathrm{L}=4\pi n^2 a\left[\log\frac{8a}{b}-\tfrac{1}{2}+\frac{b^2}{32a^2}\left(\log\frac{8a}{b}+\tfrac{1}{4}\right)\right] \quad . \quad . \quad . \quad (12).$$

Again, for a circular section of radius ρ,

$$\mathrm{L}=4\pi n^2 a\left[\log\frac{8a}{\rho}-\tfrac{7}{4}+\frac{\rho^2}{8a^2}\left(\log\frac{8a}{\rho}+\tfrac{1}{3}\right)\right] \quad . \quad . \quad . \quad (13).$$

In all these cases we see that the correction increases the value of L, and there can be no doubt that the same is true for the double coil.

I have applied (13) to estimate the correction for curvature in the self-induction of each part of the double coil. For reasons which it would take too long to explain, I arrived at the conclusion that the value of the small term must be very nearly the same for a circular section as for a square section of the same area, and the actual section is nearly enough square to allow of the use of this principle. The necessary addition to the originally calculated self-induction of each part, in order to take account of curvature, comes out 119·5 metres; so that the final value of L for the double coil will on this account be increased 239 metres. This is a small quantity, but a much larger correction for curvature must be expected in the mutual induction of the two parts. By a sufficiently approximate method I find as the correction to twice the mutual induction 3,469 metres, giving altogether for twice the mutual induction 149,289 metres. This added to 302,159 (=301,920+239) metres gives as the final calculated value of L for the double coil,

$$L=451{,}448 \text{ metres.}$$

This result is confirmed by calculation of the mutual induction by means of a table, founded on elliptic functions. In this way, and with a suitable formula for quadrature, we find,

$$2M=149{,}394 \text{ metres,}$$

agreeing nearly enough with the value found by Maxwell's method, viz., 149,289 metres.* When all the evidence is taken into consideration, there can remain, I suppose, little doubt that the value 451,000 is substantially correct, and that the reductions of the Committee are affected by a serious under-estimate.

Professor Rowland, in ignorance apparently of Maxwell's previous calculation, has shown that if in the original experiments we assume an unknown cause of error proportional to the square of the speed, and eliminate it, we shall arrive at a value of the ohm differing very appreciably from that adopted by the Committee. In this way he finds that—

$$1 \text{ ohm}=\cdot 9926\frac{\text{earth quadrant}}{\text{second}}.$$

Rowland is himself disposed to attribute the error to currents induced in the frame. Our experiments prove these currents had not much effect, though they may explain the difference between the value of L which best satisfies our experiments (where the currents could not exist), *i.e.*, 451,000, and the higher value 457,000 calculated by

* The arithmetical calculations were made from the data given by the Committee (Reprint, p. 115), $a=\cdot 158194$, $2b'=\cdot 03851$, $b=\cdot 01841$ (not ·1841), $c=\cdot 01608$, all in metres. $n=313$. The whole number of turns (313) was supposed to be equally divided between the two parts.

Maxwell as most in harmony with the original experiments. The process adopted by Rowland is evidently equivalent to determining the coefficient of self-induction from the deflections themselves, and his result, rather than that given by the Committee, must be regarded as the one supported by the evidence of the original experiments.

Rowland's own determination, by a wholly distinct method, gives—

$$1 \text{ ohm} = \cdot 9911 \frac{\text{earth quadrant}}{\text{second}};$$

and according to our experiments the ohm is even smaller—

$$1 \text{ ohm} = \cdot 9893 \frac{\text{earth quadrant}}{\text{second}}.$$

The question, therefore, arises whether any further explanation can be given of the different result obtained by the Committee. The value of GK employed in calculating the experiments according to (4) was—

$$GK = 299{,}775 \text{ metres.}$$

For the principal term in GK, as given by (7), we require the values of n, a, and α. From p. 115 of the Reprint we find $a = \cdot 158194$ metre, $n = 313$. The angle α must be recalculated, as the value of $\log \sin^3 \alpha$ (1·9624955) is evidently incorrect. From $2b' = \cdot 03851$ metre by means of $\sin \alpha = a/\sqrt{(a^2 + b'^2)}$ we find $\log \sin^3 \alpha = \bar{1}\cdot 99043$. From these data the final value is—

$$GK = 299{,}290 \text{ metres,}$$

differing appreciably from that used by the Committee. The further discussion of the question is a matter of difficulty at this distance of time. There may have been some reason for the value adopted, which it is now impossible to trace, so that I desire to be understood as merely throwing out a suggestion with all reserve. But I think it right to point out a possible explanation, depending upon the interchange of the axial and radial dimensions in the paper on the electro-magnetic field. The data there given are the mean radius, the two dimensions of the sections, and the distance between the coils (·02010). This distance is correct, being equal to $2b' - b$, that is, to ·03851 − ·01841. The distance between the mean planes of the coils is not given, but could, of course, be calculated by addition of ·02010 and ·01841. If, however, the radial dimension ·01608 were substituted for the axial dimension ·01841, an erroneous value would be obtained for $2b'$, that is, ·03618 instead of ·03851. Using ·03618 to calculate α, I find—

$$GK = 299{,}860 \text{ metres,}$$

agreeing much more nearly with the value used in the reductions.

If it be thought probable that the value of GK was really 299,290,

a still further reduction of nearly two parts in a thousand must be made in the number which expresses the ohm in absolute measure, and we should get—

$$1 \text{ ohm} = \cdot 9910 \frac{\text{earth quadrant}}{\text{second}},$$

coinciding practically with the value obtained by Rowland from his own experiments.

In the course of our experiments various doubts suggested themselves, and were subjected to examination. It may be well to say a few words about some of these, though the results are for the most part negative.

The energy of the currents circulating in the coil is expended in heating the copper, and a rise of temperature affects the resistance. Calculation shows that the disturbance from this cause is utterly insensible. If at the highest speeds of rotation all the heat were retained, the rise of temperature would be only at the rate of $3 \cdot 2 \times 10^{-8\circ}$ C. per second.

Much more heating may be looked for during the operation of taking the resistance. Under the actual circumstances a rise of resistance of about one part in 30,000 might be expected as the effect of the battery current in one minute. The aggregate duration of the battery contact in each of the resistance measurements was probably less than a minute.

Another question related to the possible effect of a want of rigidity in the magnetism of the needle. It is known that galvanometers will sometimes, when it is certain that there is no average current passing through the coils, show a powerful effect as a consequence of fluctuating magnetism corresponding to the fluctuating magnetic field. In the present experiment the magnetic field is fluctuating, and the magnet is expected to integrate the effect as if its own magnetism were constant. It is unlikely that any appreciable error arises in this way, as I find by calculation that a theoretically soft iron needle would point in the same direction as a theoretically hard needle when placed at the centre of the revolving coil.

From the details given the reader will be in a position to judge for himself as to the accuracy of our experiments. If, as we believe, the principal error to be feared is in the measurement of the coil, there is little to be gained by further experimenting with the present apparatus. Accordingly a new apparatus has been ordered, from which superior results may be expected. In designing this several questions presented themselves for solution.

All corrections being omitted, the effect—

$$\tan \phi \propto \frac{na^2 \omega}{R};$$

and, if σ denote the section of the wire, and $S(=n\sigma)$, the aggregate section of the coil—

$$R \propto \frac{na}{\sigma} \propto \frac{n^2 a}{S};$$

so that if S be given, tan ϕ is independent both of the number of terms n and of the mean radius a. If ϕ be given, the correction for self-induction depends upon L/GK, while both L and GK vary approximately as $n^2 a$. So far, therefore, there is nothing to help us in determining n and a. The following considerations, however, tell in favour of a rather large radius :—

(1.) Easier measurement of coil.

(2.) Smaller correction for moment of suspended magnet.

(3.) Smaller errors from maladjustment to centre, and from size of magnet.

The question of insulation is important. During the rotations the electromotive force acts independently in every turn, and there is no strain upon the insulation; but in taking the resistance, when a battery is employed, the circumstances are materially different. Any leakage from one turn to another would, therefore, be a direct source of error. It is proposed to use triply covered wire.

In order to obtain room for the tube encasing the fibre, it is necessary to use a double coil. In the new apparatus there will be opportunity for a much larger diameter, by which it is hoped to obtain an advantage in respect of stiffness; but the further question presents itself, whether the interval between the coils should be increased so as to obtain a very uniform field, as in Helmholtz's arrangement of galvanometer. The advantages of this plan would be considerable in several respects, but on the whole I decided against it mainly on the ground that it would magnify the errors due to imperfect measurement. If we call the effect (so far as it depends upon the quantities now under consideration) u, we have, in previous notation,

$$u = a \sin^3 \alpha = a^4 (a^2 + b'^2)^{-\frac{3}{2}},$$

$$\frac{du}{u} = \frac{4da}{a} - 3\frac{a da + b' db'}{a^2 + b'^2}.$$

If $b'=0$,

$$\frac{du}{u} = \frac{da}{a},$$

but if, as in Helmholtz' arrangement, $b' = \frac{1}{2}a$,

$$\frac{du}{u} = \frac{8}{5}\frac{da}{a} - \frac{6}{5}\frac{db'}{a}.$$

The increase of b' from 0 to $\frac{1}{2}a$ not only introduces a new source of

error in the measurement of b', but also magnifies the effect of an error in the measurement of a. If $b'=\frac{1}{10}a$, we have nearly

$$\frac{du}{u}=\frac{da}{a}-\frac{3}{10}\frac{db'}{a},$$

showing that an absolute error in b' has about $\frac{1}{3}$ of the importance of an equal absolute error in a.

As will be evident from what has been said already, the treatment of the correction for self-induction is a very important matter. It is probable that L may be best determined from the deflections themselves with the use of sufficiently varied speeds. If L be arrived at by calculation, or by independent experiments, it is important to keep down the amount of the correction. We have seen, however, that L/GK is almost independent of n, a, and S, so that if we regard tan ϕ as given, the magnitude of the correction cannot be controlled so long as a single pair of coils is used. An improvement in this respect would result from the employment of two pairs of coils in perpendicular planes, giving two distinct and independent circuits. In virtue of the conjugate character, the currents in each double coil would be the same as if the other did not exist, and the effects of both would conspire in deflecting the suspended magnet. This doubled deflection would be obtained without increase of the correction for self-induction, such as would arise if the same deflection were arrived at by increasing the speed of rotation with a single pair of coils. A second advantage of this arrangement is to be found in the production of a field of force uniform with respect to time.

However the correction for self-induction be treated, it is important to obtain trustworthy observations at low speeds. In order to get a zero sufficiently independent of air currents, it will be advantageous largely to increase the moment of the suspended magnet. Preliminary experiments have, however, shown that there is some difficulty in getting the necessary moment in a very small space, in consequence of the interference with each other of neighbouring magnets, and thus the question presents itself as to the most advantageous arrangement for a compound magnet.

A sphere of steel, as used by the Committee, has the advantage that if uniformly magnetised it exercises the same action as an infinitely small magnet at its centre. But the weight of such a sphere is considerable in proportion to its moment, and it is probable that a combination of detached magnets is preferable. It is possible so to choose the proportions as to imitate pretty closely the action of an infinitely small magnet. Thus, if the magnet consist of a piece of sheet steel bent into a cylinder and uniformly magnetised parallel to the axis, the length of the cylinder should be to the diameter as $\sqrt{3}$ to $\sqrt{2}$. In this case the action is the same as of an infinitely small magnet as far

as the fourth term inclusive of the harmonic expansion. Without loss of this property the cylinder may be replaced by four equal line magnets, coinciding with four symmetrically situated generating lines. Thus, if we make a compound magnet by placing four equal thin magnets along the parallel edges of a cube, the length of the magnets should be $\sqrt{3}$ times the side of the cube. This is on the supposition that the thin magnets are uniformly magnetised, as is never the case in practice. To allow for the distance between the poles and the ends of the bars, we may take the length of the bars 2·3 times the side of the cube.

With the new apparatus, and with the precautions pointed out by experience, we hope to arrive at very accurate results, competing on at least equal terms with those obtained by other methods. Most of the determinations hitherto made depend upon the use of a ballistic galvanometer, and the element of time is introduced as the time of swing of the galvanometer needle. There is no reason to doubt that very good results may be thus obtained, but it is, to say the least, satisfactory to have them confirmed by a method in which the element of time enters in a wholly different manner.

Part II.—By Arthur Schuster.

Adjustment of the Instruments and Determination of Constants.

The only adjustments to be made consist in—

1. The levelling of the coil.
2. The suspension of the magnet in the centre of the coil.
3. The proper disposition of the scale and telescope by means of which the angles of deflection are read off.

Level.—The first of these presents no difficulty, and any small error can be easily taken account of in the calculations. It was found that the upper end of the axis of rotation was inclined towards the north by an angle of ·0003 circular measure. Hence, as has already been explained (page 107) we must in the reductions write throughout G (1 + ·0003 tan I) or 1·0008 G for G. This correction is small, but a little uncertain, as the coil was not very steadily fixed in its bearings, and small variations in the inclination of the axis could be produced by slightly pressing on one side or the other of the coil. When left to itself the coil seemed, however, very nearly to return to the same position.

The Magnet.—The magnet, which was suspended in the centre of the coil, consisted of four separate magnetised needles, each about 0·5 centim. long. These were mounted on four parallel edges of a small cube of cork. A needle attached to the back of the mirror went through a small hole in the cork, and was kept in its place by means

of shellac, to prevent any slipping between the magnets and the mirror. The proper suspension of the magnet is a point of some delicacy and importance. As regards the vertical adjustment, the distance of the cube of magnets from the highest and lowest point of the circular frame was measured, and the magnet raised or lowered until the distances became equal. A pointer was next fixed to the frame, reaching very nearly to the centre of the coil. As the coil was rotated, the pointer described a small circle round the axis of revolution, and the position of the magnet could be easily altered until it occupied the centre of the small circle. It is supposed that this adjustment was made to within less than 1 millim., and could, therefore, for all practical purposes, be considered as perfect. The magnetic moment of the magnet was measured in the usual way. Two closely agreeing sets of measurement showed that at a distance of 1 foot it deflected a suspended needle through an angle, the tangent of which was ·000298. Hence at the mean distance of the coil (15·85 centims.) the deflection would have been ·0021. This number is equal to $\frac{MK}{GH}$, and will be referred to as $\tan \mu$ in the discussion of the calculations. The magnetic moment was determined a few days after the last spinnings had been taken; but on each day on which experiments were made, the time of vibration of the magnet was determined, and we thus assured ourselves that no appreciable change in the magnetic moment had taken place while the experiments were going on. The time of one complete vibration was 14·6 seconds.

Adjustment of Scale and Telescope.—The telescope which served to read the angle of deflection rested on a small table to which it could be clamped. In front of the table and below the telescope, the scale could be raised or lowered and fixed when the proper position had been found. It was levelled by deflecting the magnet successively towards both sides, and observing the point of the scale at which the cross wires of the telescope seemed to cut the scale. If in both positions of the mirror the scale was intersected at the same height, it was considered to be sufficiently levelled. It remained to place the scale at right angles to the line joining its centre to the mirror. This was done by measuring the distance of both ends to the mirror by means of a deal rod, with metallic adjustable pointers (presently to be described), and altering the position until these distances were equal. It is supposed that considerable accuracy was thus obtained. A small remaining error would be eliminated by observing deflections on both sides of the zero. To adjust the telescope we had now only to point it to the centre of the mirror, and at the same time to place it in such a position that its optic axis passed vertically over the centre of the scale. By suspending a plumb line from the telescope so as to divide

its objective into two equal parts, and focussing alternately on the mirror and on the image of the scale, both points could be simultaneously attended to.

To measure the distance of the scale from the mirror the deal rod used for the adjustment of the scale was cut down so as to have nearly the required length. The two brass pointers attached to the two ends made an angle of about 45° with the rod. One of the pointers was fixed but the other could be moved round a fixed point in the rod by means of a screw. As it moved the distance of the two points changed, and by properly supporting the rod and leaning one point against the centre of the scale at a known height from the ground, while the moveable point was made to touch the centre of the mirror, the distance could be accurately found. It was compared with the scale itself, in order that the calculation of the angles of deflection should be independent of the absolute length of a scale division. The length required is the shortest line between the centre of the mirror and the plane of the scale, and this can be calculated if the difference in height of the centre of the mirror and the point to which the distance was measured, is known. These heights were determined by means of a cathetometer. The height of the centre of the objective was measured at the same time; so that all data required to find the inclination of the normal of the mirror to the horizontal are known. The following numbers were obtained; each division of the scale is for simplicity supposed to be equal to 1 millim., which is very nearly correct, but as has been said, its absolute value is of no importance.

Distance of mirror from scale in centims.	252·28
As the position of the magnet was always read off through a glass plate, a small correction equal to the thickness of the glass (3·2 millims.) multiplied into $1-\frac{1}{\mu}$, where μ is the refractive index, has to be applied. This correction is subtractive and equal to	0·11
Hence, D=	252·17

It was also found that the mirror pointed downwards, and made an angle of ·004 with the horizontal. A small correction due to this cause will be discussed in another place.

Torsion.—The torsion was as much as possible taken out of the silk fibre, which was about 4 feet long, before the magnet was attached to the mirror. The coefficient of torsion was determined by turning the magnet through five whole revolutions and observing the displacement of the magnet. It was calculated from the numbers obtained that

one revolution shifted the position of rest through 5·6 scale divisions, corresponding to an angle of ·001107.

Another experiment in which the magnet was turned in the opposite direction gave ·001117.

Hence
$$\tau = \frac{\cdot 00111}{2\pi} = \cdot 00018.$$

The correction due to torsion is best applied to the value of G at the same time as the correction for level by writing everywhere—

$$G\,\frac{1+\beta\tan I}{1+\tau} \text{ for } G$$

Constants of the Coil.—The accurate determination of the constants of the coil forms the most difficult part of the measurements. Unwinding the coil, the outer circumference of the successive layers was measured by means of a steel tape. Each measurement was repeated several times, and the agreement was satisfactory. The thickness of the wire was found to be 1·37 millims., which, on the circumference of the successive layers, should produce a constant difference of 2·74 π or 8·62 millims. Owing, however, to defective winding, each layer enters a little into the grooves of the subjacent layer, and the differences in circumference of successive layers were therefore always smaller than they ought to have been. The differences varied between 7·7 millims. and 8·6 millims., but generally were about 8·1 millims. The wire was a little too thick, and as it had been taken off the coil and rewound, small irregularities were formed which prevented a satisfactory winding. Each coil had 156·5 windings. Of these 156 were in one coil regularly distributed over twelve layers of thirteen windings each; while half a turn was outside. In the second coil the twelve layers only contained 155 windings, and one turn and a half was lying outside. In the calculation for mean radius it was assumed that each complete layer contained the same number of turns. Let S be the sum of all measurement for one coil, also C the circumference of the layer containing the loose extra turns; then we find the mean circumference of the first coil, $\frac{13S+0\cdot 5\,C}{156\cdot 5}$ .... = 99·680

and for the second, $\frac{(13-1/12)S+1\cdot 5\,C}{156\cdot 5}$ .... = 99·651

Or as the circumference of the outside of the mean turn. = 99·666

From this is to be subtracted a correction equal $\pi \times$ thickness of tape .... = ·031

99·635

To obtain the circumference of the axis of the mean winding we have to subtract $\pi \times$ thickness of wire .... =	·431
Hence the final value of the mean circumference ...... β=	99 ·204
Or for the mean radius .... a=	15 ·789

The grooves of the coils and their distance was also measured, and it was found that—

b=axial dimension of coil.................... =	1 ·833
b'=distance of mean plane from axis of motion... =	1 ·918

All measurements are given in centimetres.

We calculate—

α=angle subtended at axis by mean radius $=\cot^{-1}\frac{b'}{a}$... =	83° 04′
And $\log \sin^3 \alpha$ .... =	$\bar{1}$ ·990458
The principal term in the expansion of GK is $\pi n^2 \beta \sin^3 \alpha$=	29,869,300
To this is to be added a small correction given on p. 107 =	100
The final value of GK being	29,869,200

Applying the corrections for level and torsion to GK as explained, and writing $\mathfrak{GK}$ for the value so corrected, we find,

$$\mathfrak{GK}=29,887,600.$$

The Observations.

The observations consisted of two parts: the comparison of the resistance of the rotating copper coil with that of a standard German silver coil, and the observation of the deflections during the spinnings. The comparison of resistance was made by a resistance balance devised by Mr. Fleming,* to whom we are indebted for advice and assistance in all questions concerning the comparison of resistance. In this balance, which only differs by a more convenient arrangement from an ordinary Wheatstone's bridge, Professor Carey Foster's method of comparing resistance is used. The method consists in interchanging the resistance in the two arms of the balance which contain the graduated wire, and thus finding the difference between these two resistances in terms of that of a certain length of the bridge wire. The balance was placed on a table near the rotating coil, and could be electrically connected with it by means of two stout copper rods. The German silver coil which served as the standard of comparison was prepared so as to have a resistance nearly equal to that of the copper coil, that is about 4·6 ohms. Any error due to thermo-electric currents, which have sometimes been found to be generated at the

* "Phil. Mag.," ix, p. 109, 1880.

moveable contact of the galvanometer circuit with the bridge wire, is eliminated in Foster's method, but to ensure greater accuracy and safety all measurements were repeated with reversed battery current. The whole comparison seldom occupied more than five minutes; and the readings obtained with the battery current in different directions closely agreed with each other.

The spinnings were always taken in sets of four at the same speed, and the comparison of resistance was made at the beginning and end of each set. During the time of spinning the resistance was found to have altered owing to a rise of temperature which always took place during the time of experimentation. The corrections for the change of resistance and the possible errors introduced owing to the uncertainty of this correction will be described further on.

After the resistance of the coil had been measured, it was disconnected from the balance and set into rotation with open circuit, so that no current could pass. While the water supply was adjusted so as to give approximately the required speed, the magnet in the centre of the coil, which had been strongly disturbed during the measurement of resistance, was brought to rest either by means of an outside magnet or more often by means of a small coil and Le Clanché cell, which was always placed in the neighbourhood of the rotating coil. A key within reach of the observer served to make and break contact at the proper time. When the speed had been approximately regulated and the magnet was vibrating through a small arc only, its position of rest was determined, while at the same time the auxiliary magnetometer placed in the adjoining room was observed. The two ends of the rotating coil were now connected together, by means of a stout piece of copper, the well amalgamated ends of which were pressed into cups containing a little mercury, into which they tightly fitted.

As the coil was set into rotation the magnet slowly moved towards one side, and a proper use of the damping coil brought it quickly to approximate rest near its new position of equilibrium. When the swings extended through no more than ten or twenty divisions of the scale, the coil was kept, as nearly as possible, at the proper speed, by the observer at the tuning fork (Lord Rayleigh, see p. 112). Readings of the successive elongations were taken for about two minutes, and a signal given at the beginning and end of each set of readings enabled the observer at the auxiliary magnetometer (Mrs. Sidgwick) to note its position as well as any changes in the direction of the earth's magnetic force during the time of observation. The direction of rotation was now reversed, and the deflection observed in the same manner; the whole process being twice repeated, so that four sets of readings were obtained. When all the observations for the given speed had been completed, the position of rest of the magnet, when no current passed through the coil, was again determined and compared

with the auxiliary magnetometer. A recomparison of resistance with the standard completed the set. The magnet in the centre of the coil should, when no current is passing through the coil, always go through the same changes as the magnet of the auxiliary magnetometer. If this could be insured, the two might be compared once for all, or the comparison might even be omitted altogether, for the difference between the deflections of positive and negative rotations, when corrected for changes in the earth's magnetism, would give the double deflection independently of the actual zero position. Unfortunately, however, and this was our greatest trouble, the comparison between the magnet and the auxiliary magnetometer showed that we had to deal with a disturbing cause, which rendered a frequent comparison between the two instruments necessary. This disturbing cause, which we traced to air currents circulating in the box containing the magnet, will be discussed presently.

The observations were taken on three different evenings and one afternoon. The evenings (8 h. P.M. to 11 h. P.M.) were chosen on account of the absence of disturbances, which, during the usual working hours, are almost unavoidable in a laboratory. We may give, as an example for the regularity with which the magnet vibrated round its position of rest, a set of readings which were taken while the coil revolved about four times in one second, the circuit being closed.

$T = 9^{h}\ 36^{m}$. $t = 13°·0$ C.

	Rotation.		Negative.
	374·4	....	362·1
	373·3	....	362·8
	372·2	....	362·0
	373·9	....	361·4
	372·8	....	362·0
	372·8	....	362·0
	372·4	....	363·8
	371·8	....	364·0
	371·1	....	364·0
	370·5	....	
Mean....	372·52	....	362·68

Position of rest, 367·60.

$T = 9^{h}\ 38^{m}·5$. $t = 13°·0$ C.

The number of readings taken were not always the same, but varied generally between sixteen and twenty.

We used, in the course of our experiments, four different speeds. The method of obtaining and regulating these has been explained by

Lord Rayleigh. For simplicity we generally denoted the speed by means of the number of teeth on the circle which seemed stationary when looked at through the tuning fork; thus we spoke of a speed 24 teeth, 32 teeth, and 60 teeth. To obtain the lowest speed the circle containing 60 teeth was looked at over the top of the tuning fork, so that only one view for each complete vibration was obtained; this was equivalent to a circle of 120 teeth in the ordinary arrangement, which allowed a view for each half vibration, and, consequently, the lowest speed was called 120 teeth. The velocity of rotation depends, of course, on the frequency of the fork, which varied only within narrow limits, and was always very near 63·69. If f denote this frequency and N the number of teeth on the stationary card, the velocity of rotation ω is given by the equation $\omega=4\pi f/N$. In the "British Association Report" the speed is always indicated by means of the time occupied by 100 revolutions. If T is this time, we find $T=50N/f$. The following table gives the comparison of ω, T, and N, on the supposition that the frequency of the fork was always the same and equal to 63·69.

N.	ω.	T.	Number of turns per second.
120	6·670	94·206	1·06
60	13·339	47·103	2·12
32	25·011	25·122	3·98
24	33·348	18·841	5·31

The last column gives the number of turns per second.

Three speeds were taken on each of the three nights, and one set of observations with the lowest speed was secured in the course of one afternoon. We obtained in this way three sets for each of the two intermediate speeds and two sets for the lowest and highest speed. A comparison with the Report of the British Association Committee shows that we do not go up quite to their highest speeds, but that on the other hand our lowest speed was considerably below the one used by them. In the Report for the year 1863, it is mentioned that the forced vibration of the magnet, depending on the rotation of the coil, could not be noticed, and it is calculated that the amplitude of this vibration was less than $\frac{1}{100}$ of a millimetre on the scale. No mention is made of this forced vibration in the Report for 1864, although much lower speeds were used during that year. In our lowest speed a slight shake of the needle, due to the varying action of the currents in the coil, was distinctly seen; but as calculation showed that the amplitude was only the eighth part of a millimetre on the scale, no appreciable error is supposed to have been introduced by it. The moment of inertia of the suspended parts was higher in the experiments made by the British Association, and this,

no doubt, is partly the reason why this forced vibration escaped their notice.

Air Currents.

It has already been noticed that air currents in the box containing the magnet affected its position to some extent, and we had to investigate in how far our final results might be affected by this disturbance. During the first night (December 2) our attention had not been drawn so much as it was afterwards to the effect of these air currents. We had previously ascertained, by a series of careful measurements, that the rotation of the coil with open circuit did not sensibly affect the zero position of the magnet, and we considered it sufficient to note the zero as short a time as possible before each set of four spinnings. The comparison of these zeros with the auxiliary magnetometer showed that during the two hours of experimenting, the needle had kept its zero within two divisions of the scale, so that the changes during two successive spinnings (generally about five minutes) must have been very small. On the second night (December 6) however, the zeros were taken at the beginning and end of each set of four spinnings, and the disturbance due to air currents was found to be of more importance. The following table reveals the fact that during a set of spinnings the magnet seems to have moved in one direction, but that during the time the coil was at rest and the comparison of resistance was made, it went in the opposite direction. The numbers given are corrected for changes in the direction of the earth's magnetic force as shown by the auxiliary magnetometer.

December 6.

Number of teeth on stationary circle.	Time. h. m.	Position of rest.	Approximate deflection.
60	8 53	763·60	218
	9 12	766·35	
32	9 31	764·88	397
	9 56	765·78	
24	10 9	762·67	514
	10 38	766·48	

Here, then, we have a gradual rise in the zero from one to over three divisions during a set of four spinnings. The approximate deflection is given in order to give an idea what amount of error the uncertainty of the zero might introduce.

Special experiments were now made, and it was found that by placing a lamp about a foot and a-half from the magnet box, changes amounting to eighteen divisions of the scale would be observed;

greater precautions were taken, in consequence of the experience thus gained, to secure the box from the radiation of the lamp and gas jets, which could not be dispensed with in the course of the experiments. The magnet box was covered with gold-leaf so as to reflect the heat as much as possible. On the last night of work frequent determinations of the position of rest were made, but in spite of all precautions an unknown cause produced a sudden displacement of five scale divisions. The exact time at which this change took place could not be determined, and two spinnings were therefore rejected. During the remainder of the evening the magnet gradually came back to its original position. With the exception of the two spinnings just mentioned we have not rejected any observations.

When we come to inquire into the amount of uncertainty to which our results are liable, owing to the effects of these air currents, we find that it cannot be greater than the more dangerous, because less evident, errors to which the determination of our constants (mean radius and distance of mirror from scale) are subject. As long as the changes of the position of rest take place irregularly, the error would tend to disappear in the mean, and the probable error deduced from our experiments would give a fair idea of the uncertainty due to this cause. This probable error, as we shall see, is very small. A regular displacement of zero in one direction would, however, produce a constant error which would not disappear in the final mean. We have some evidence that such a regular displacement has to some extent taken place. The comparison of zeros on December 6, as quoted above, for instance, shows the position of rest in the course of the spinnings shifted towards increasing numbers. Such a shift, if not taken into account, would tend to make the deflections towards increasing numbers (positive rotation) appear larger than those towards decreasing numbers. This, indeed, was observed. Supposing the shift takes place regularly during the time of spinnings we might have taken it into account. But the correction which we should have had to apply is so small and uncertain that it is doubtful whether we should have improved our final result, and it would certainly not have altered it within the limits within which we consider it accurate; for we find that reducing the deflections on the supposition, 1st, that the zero has kept constant; and 2nd, that it has changed uniformly during each set of spinnings; the two results agree to within about one and a-half tenths of a division, which, even at the lowest speeds, would only make a difference of about 1 in 750, and on the highest speeds four times less. The fact that a regular shift in the zero position of the magnet affects the mean of four spinnings is due to the arrangement of experiments, adopted during the first two nights, in which four rotations succeeded each other in alternate directions. If, after a rotation in the positive direction, two negative rotations,

followed again by a positive one, had been taken, a regular displacement such as that we are discussing would not have affected the mean. This latter plan was adopted on the last night. In the measurements undertaken by the British Association Committee, the deflections in one direction were generally greater than in the other, and the difference was ascribed to a considerable torsion in the fibre of suspension, which, in order to explain the discrepancy, must, as pointed out by Rowland, have displaced the magnetic axis considerably out of the meridian. The differences in the readings taken when the coil was spinning in opposite directions were, on the average, about 3 per cent., and amounted in one case to 8 per cent. They show no regularity dependent on the speed of rotation. We also observed some slight differences of the same nature; but they are very small, and always remain within such limits that they may easily have been produced by a displacement due to air currents. On the last night, when more frequent zero readings were taken, the differences were, it is true, not much reduced in amount, but their sign was reversed. It is, perhaps, worth remarking that, owing to the absence of any controlling instrument equivalent to our auxiliary magnetometer, the Committee of the British Association had no opportunity of discovering the presence of these air currents, as any changes in the zero position would naturally have been ascribed by them to a casual change in the direction of the earth's magnetic force. Owing to the different shape and material of the box containing the mirror, it seems possible that the effect of air currents may have been considerably larger than it has been in our experiments.

Reduction of Observations.

Scale Corrections.—The first step in reducing the observations consists in calculating the value of $2\tan\phi$ from the observed deflection. The correction to be applied to the reading in order to obtain numbers proportional to the tangents of deflection, if the position of rest of the magnet is at the centre of the scale, would be $-d^3/4D^3$; d being the observed reading, and D the distance of the mirror from the scale. If the zero, however, is at a point δ of the scale, the correction becomes $-(d-\delta)(d+\delta)^2/4D^2$, where δ is to be reckoned positive when in the same direction as d. For the higher speeds a second correction, to $+d^5/8D^4$, was applied, which comes just within the limits of accuracy aimed at in the actual readings. The corrected deflections so obtained are equal to $2D\tan\phi$. Small errors, due to a faulty division of the scale, ought also to be applied. It is difficult to obtain a proper scale in one piece of sufficient length to be used in these experiments; and the one in actual use consisted of three parts, cemented with caoutchouc cement to a thick piece of deal. No appre-

ciable error was introduced by a very slight warping of the wood, and the scales were found to be very accurately divided, but the small errors existing were corrected; small corrections had also to be introduced, which are due to the imperfect joining of the different pieces. The combined correction never amounted to more than ·15 of a division. Each division, as has already been stated, being very nearly equal to 1 millim.

It has already been noticed that the normal to the mirror pointed slightly downwards. The correction due to this want of adjustment seems to have been generally neglected, yet it is not altogether unimportant. If p is the vertical distance between the centre of the objective and that point of the scale where it is cut by the normal to the mirror; also if α is the inclination between the normal to the mirror and the horizontal, the correction to be applied to a deflection d is $dp\alpha/\mathrm{D}$, where D is the distance of the mirror from the scale. In our experiments the correction amounted to $d \times 0{\cdot}00014$, although the angle between the normal and the horizontal was only about 14 minutes of arc. The correction is positive only if the normal lies between the horizontal through the mirror and the line joining the mirror to the cross wires of the telescope.

Correction for Temperature.—We have now to discuss a series of corrections which have to be applied in order to make a comparison of the results obtained in different spinnings possible. It has already been noticed that four spinnings at the same speed were always taken into one set. The comparison of resistance at the beginning and end of each set showed that during the time of spinning the temperature had altered; before combining the mean within each set we had, therefore, to correct for the change of temperature. We endeavoured to keep the room as much as possible at a constant temperature during the experiments; the lamps used were always lighted nearly two hours before beginning, but, in spite of all precautions, the temperature always rose after we had entered the room and begun to work. The thermometer rose at first pretty rapidly through about 1 degree, and then rose slowly until at the end of the evening it stood generally nearly 2 degrees higher than at first. When the first set of spinnings commenced, the rapid rise, as shown by the thermometer in the room, had already subsided; but, as was to be expected, the temperature of the coil was lagging somewhat behind that of the room, and its resistance still rose appreciably. Thus, during the first night, the resistance of the copper coil rose almost ·4 per cent. during the course of the first set of four spinnings. If the curve of temperature of the coil is known, there is of course no difficulty in applying the proper correction. This curve can be obtained approximately by plotting down the measured resistances as ordinates with the time as abscissæ. This was done for all observations made on December 2; but during the

succeeding nights it was found that the curve could not be sufficiently well determined by the observations, and that the assumption of a uniform rise in resistance during the time elapsing between two successive measurements would give as good results as the experiments themselves would allow us to obtain. The proper determination of this correction is a subject to which we shall have to give some attention in the more accurate measurements which we have in view. At present it will suffice to point out that, as far as we can judge, the error due to uncertainty of temperature is not more than ·05 per cent. during the first set of spinnings on each night. It is much smaller in the succeeding sets. It may increase the clearness of this account if at this point we give a specimen, worked out in detail, of one set of deflections. Let the resistance of our standard German silver coil, which we always have assumed to be at the temperature of the air, be called S, and the resistance of the rotating coil C. A comparison by means of the balance shows that

$$C = S + a,$$

where a is the resistance of a certain length of the bridge wire, differing slightly at the beginning and end of the experiment.

December 6.

Number of teeth on stationary circle, 32.

Comparison of resistance, $C = S + \cdot 0225$. Time $= 9^h\ 17^m$.
Position of rest .... 766·48. Time $= 9^h\ 32^m$.
Auxiliary magnetometer .... 26·9.

Time = ...	$9^h\ 37^m$	$9^h\ 42^m$	$9^h\ 47^m$	$9^h\ 53^m$
$t =$	13°·0	13°·0	13°·0	13°·0
Rotation................	negative	positive	negative	positive
Deflected reading	367·60	1166·40	366·23	1166·09
Auxiliary magnetometer..	27·55	28·24	28·50	28·30

Auxiliary magnetometer .... 27·2. Time $= 9^h\ 57^m$.
Position of rest .... 767·08.
Comparison of resistance, $C = S + \cdot 0272$. Time $= 10^h\ 0^m$.

From the comparison of zeros with the auxiliary magnetometer at the beginning and end of the experiments, we find for the corresponding readings during the experiments, 766·78 and 27·05. Considering that increased readings, if the magnet in the coil correspond to decreased readings in the auxiliary magnetometer, we find

the following numbers for the positions of rest during the experiments :—

Position of rest.........	766·28	765·59	765·33	765·53
Deflected reading.......	367·60	1166·40	366·23	1166·09
Deflections	−398·61	+400·81	−399·10	+400·56
Scale correction	+ 2·08	− 2·93	+ 2·08	− 2·94
Reduction of temperature to Time=9^h 37^m	+ 0·05	+ 0·05	− 0·21	+ 0·35
Corrected deflection..	−396·55	+397·93	−397·23	+397·97

Mean deflection...... 397·42.

C=S+0·0248.

When all the spinnings had been reduced in this way, the final calculations for the actual resistance were made. The determination of all quantities involved has been explained, with the exception of the measurement of the absolute pitch of the tuning-fork.

Rate of Vibration of Tuning-fork.—As has already been explained, the tuning-fork which was used to regulate the speed was on every night compared with a standard fork, and our determinations, therefore, all depend on the absolute pitch of this standard fork. The method used to determine that pitch has been described by Lord Rayleigh.*

A fork, vibrating about 32 times a second, maintained by means of an electric current, and driving a second fork of fourfold frequency, was compared directly with the clock. The vibrations of the driven fork were simultaneously compared with the standard by counting the number of beats in a given time. A few experiments have to be made in order to see whether the fork gains on the clock, or *vice versâ*, and also whether the standard vibrates quicker or slower than the driven fork. This can be done by gradually shifting weights on the driver. The difference in the time of vibration of the clock and driving fork was generally such as to give one cycle in between 20 or 30 seconds. The driven fork gave at the same time from 5 to 11 beats per minute.

The experiments agreed well with each other, and both the rate of vibration and the temperature variation are in close agreement with the determinations made by Professor McLeod and Mr. G. S. Clarke† of other tuning-forks which, like ours, were made by König.

The following series of determinations was made at a temperature of about 13° C. :—

* "Nature," xvii, p. 12, 1877.

† "Phil. Trans." vol. 171, p. 1, 1880.

128·179
·180
·181
·179
·174
·180
·189
·185

The small discrepancies would very likely be still further reduced if greater care was taken to ascertain the exact temperature of the fork. As a mean of different sets we find

Number of vibrations in 1 second $=128·180$ for $t=13°·0$ C.
$128·161$ $t=14°·6$ C.

From these data and the number of beats counted during each course of experiments we can, with the necessary accuracy, determine the number of vibrations of the fork, which served to regulate the velocity of the revolving coil.

Calculation of Results.—For accurate calculation, the expansion given in the Report of the British Association is not sufficient. Instead of taking into account a greater number of terms, we may with equal facility have recourse to the original quadratic equation for the resistance. We find

$$\mathrm{R}=\frac{2f\mathfrak{G}\mathfrak{K}\cot\phi}{\mathrm{N}}\left[\tfrac{1}{2}(1+\tan\mu\sec\phi)+\sqrt{\tfrac{1}{4}(1+\tan\mu\sec\phi)^2-\mathrm{U}\tan^2\phi}\right].$$

In this equation, f, as before, is written for the frequency of the electrically maintained fork, and N for the number of the teeth on the apparently stationary circle.

U is written for $$\frac{2\mathrm{L}}{\mathfrak{G}\mathfrak{K}}\left(\frac{2\mathrm{L}}{\mathfrak{G}\mathfrak{K}}-1\right).$$

The equation is correct if the torsion and deviation from level are taken into account in the value of GK as has been explained. The only approximation used in the equation is that of writing $\tan\mu$ for $\frac{\mathrm{KM}}{\mathrm{GH}}$.

Results.

The results of the calculation are collected in the following table. The first column contains the date on which the experiments were made; the second, the speed in terms of the number of teeth on the stationary card; the third column gives the deflection corrected for all scale errors and variations of temperature during each set; the fourth column shows the value of resistance in absolute measure as

obtained by calculation on the assumption that the coefficient of self-induction of the coil is $4{\cdot}51 \times 10^7$ centims. This absolute resistance refers to the German silver coil, and a small length of the bridge wire at a given temperature. As both the temperature and this length of bridge wire varied in different experiments, the different results cannot be directly compared, but we can easily apply a correction which shall reduce the numbers to the absolute resistance of the German silver coil at a fixed temperature. The temperature chosen was $11^\circ{\cdot}5$ C., which was approximately the lowest temperature observed in the course of the experiments. The fifth column contains the corrected values, which now can be compared together, and give the absolute resistance of the standard coil as observed on different occasions, and with different speeds. In the last column the mean value for the different speeds is given. In taking these, as well as the final mean, it must be observed that the set of observations made on December 10 with speed 60 teeth contained only two spins, or half the usual number.

Date.	Speed. No. of teeth on stationary card.	Deflection.	$R \times 10^{-9}$.	$R \times 10^{-9}$ corrected.	Mean.
Dec. 7...	120	110·42	4·5486	4·5419	4·5364
10...		110·22	4·5568	4·5309	
Dec. 2...	60	218·61	4·5580	4·5487	4·5467
6...		218·30	4·5620	4·5471	
10...		218·72	4·5531	4·5422	
Dec. 2...	32	397·75	4·5639	4·5417	4·5427
6...		397·39	4·5672	4·5415	
10...		397·26	4·5687	4·5448	
Dec. 2 ..	24	513·73	4·5719	4·5446	4·5442
6 ..		513·58	4·5734	4·5438	

The mean of all the observations is—

$$R = 4{\cdot}5427 \, \frac{\text{earth quadrant}}{\text{second}}.$$

The value of the self-induction which was adopted in these calculations is slightly smaller than the values calculated by Lord Rayleigh and Mr. Niven. A comparison of the results obtained with different speeds shows that the value must be very nearly correct, for there is no decided difference between the results. Nevertheless, it seemed of interest to calculate the value of the self-induction which best agreed with the experiments, and to see whether that value gave an appreciably different result for R.

We may, in fact, treat both R and L as unknown quantities, and employ the methods of least squares to find out the most probable values. We use for this purpose the approximate values already found, and find the most probable corrections to them. Neglecting the small corrections for torsion, magnetic moment, and level, and writing $P=2R/GK\omega$, we find for the quadratic which determines R—

$$P^2-P\cot\phi+U=0,$$

where U as before is written for $\frac{2L}{GK}\left(\frac{2L}{GK}-1\right)$.

We find approximately by differentiation, remembering that $dP/P=dR/R$,

$$d(\tan\phi)=-\frac{dR}{R}\left(\frac{1}{P}-\frac{3U}{P^3}\right)-\frac{dU}{P^3}.$$

We may consider dR/R and dU to be the unknown quantities to be determined. The coefficients with which they are multiplied are known with sufficient accuracy. $d\tan\phi$ is found for each observation by putting $dU=0$ and dR equal to the difference between the value of R calculated by means of that observation, and the value of R provisionally adopted. The usual methods to form the normal equations were employed. We find in this way—

$$R=10^9\times(4{\cdot}5433\pm0{\cdot}0019)$$

$$L=10^7\times(4{\cdot}5130\pm{\cdot}0110)$$

It is satisfactory to note that the final value of R derived with the aid of the theory of probability is practically identical with the mean value directly calculated from our experiments with $4{\cdot}51\times10^7$ as coefficient of self-induction. A remarkable agreement is shown between the value of this coefficient of self-induction best fitting our experiments................................ $4{\cdot}5130\times10^7$

and the value calculated from the dimensions of the coil .... $4{\cdot}5145\times10^7$

The large probable error, however, shows that the agreement is partly accidental.

To give an idea of the accuracy with which R has been determined by means of our experiments independently of constant errors, it may be mentioned that the probability of our value being wrong by one in a thousand is only one in ten, while the experiments made by the British Association give an even chance for the same deviation.

It only remains to determine the resistance of the German silver standard in ohms at a temperature of 11°·5 C.

We had at our disposal the original standards prepared by the Committee of the British Association. These are very nearly equal

at the temperature at which they are supposed to be correct, and the ohm as determined by the Committee is, of course, uncertain within the limits within which the standards differ, but for our present purpose these may be considered identical. The coils were carefully compared by Mr. Fleming, who also determined their temperature coefficient. One coil in a flat case (hence called the "flat coil"), which had the smallest temperature coefficient, and supposed to be right at 14°·8 C., was taken at that temperature as the true ohm. Six of the standards were so arranged and joined together by means of mercury cups, that four were in a row, and the remaining two in double circuit, the whole system of coils being thus equivalent to about 4·5 ohms. Our standard German silver was nearly 4·6 ohms. As the difference was too great to allow a direct comparison by means of Mr. Fleming's bridge, a piece of German silver wire was prepared so as to have a resistance of ·1 ohm; this could easily be done within the required limits of accuracy by means of a set of resistance coils belonging to the Laboratory. Having thus a set of resistances very nearly equal to the one to be measured, a series of experiments was made on two successive days. Knowing all the temperature coefficients, we could easily reduce the measurements to ohms. Four different experiments gave for the German silver standard at 8°·5 C.—

$$\begin{array}{r} 4{\cdot}5902 \\ 4{\cdot}5896 \\ 4{\cdot}5869 \\ 4{\cdot}5879 \\ 4{\cdot}5890 \\ \hline \text{Mean} = 4{\cdot}5887 \end{array}$$

Assuming the German silver to vary 4·4 per cent. for 100° C., we find for our standard at 11°·5 C. 4·595 ohms. We have hitherto neglected to take account of the resistance of the two stout copper rods which connected the rotating coil with the resistance bridge. This resistance was determined by Mr. Fleming to be ·003 ohm. To make matters equal, we ought to have added that resistance to the British Association standards in comparing them with the standard used by us, and we should then have found that the absolute resistance found by us to be equal to $4{\cdot}543\,\dfrac{\text{earth quadrant}}{\text{second}}$, was equal to 4·592 ohms.

From this we calculate that the ohm as fixed by the Committee of the British Association is—

$$\cdot 9893\,\frac{\text{earth quadrant}}{\text{second}},$$

this being the final result of our experiments.